

difficult reading. Goodman's book is very much in the new style. The result, however, is a study that does justice to John of Gaunt at last. Due to an explosive combination of social unrest, unsuccessful foreign war, fear of invasion, and, it must be said, the abrasive characters of many of those involved, Gaunt had to work in a period of particularly beastly and violent politics. In this connection Goodman might have made more use of the *Westminster Chronicler*, who vividly described the generally vicious atmosphere.

Goodman makes it amply clear that Gaunt's activities in the Iberian Peninsula were due as much to support of English state policy as to his own personal ambitions. His lack of success in both spheres was due to the fact that by this time English pretensions abroad were desperately beyond the combined financial resources of both the monarchy and Gaunt himself. These resources were often desperately entangled, so much so that the duke was generally owed large sums of money by the crown. Other verdicts offered by Goodman are extremely interesting. Gaunt's early support of Wycliffe was not entirely opportunistic. In a rather confused way he admired the dissident's austerity, while at the same time also admiring the ultra-orthodox Carmelites and the growing cult of the Blessed Virgin, and yet unashamedly exploiting his own position as a lay patron.

Gaunt's clumsy handling of the political situation of 1376-77 left him with an evil reputation that took him a long time to live down, although thereafter his influence was always cast on the side of political reconciliation. In the end, his relations with the rest of the higher nobility were a qualified success; and he avoided polarizing the peerage into pro and anti-Lancastrian factions, and he was remarkably successful in avoiding regional conflicts with them. Moreover, although some of his most acrimonious disputes were with gentry neighbors and tenants, his well-publicized abhorrence of the abuses of maintenance meant that the conduct of his own huge retinue was never a matter of general complaint. And could anyone have maintained a successful relationship with someone as obnoxious as Richard II?

The book also clearly brings out how heavily dependent the late medieval army was on magnate power and suggests that the magnates' participation in war increased the activity of their private government in society and added to their power and influence. Altogether, this is a book well worth serious study.

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PHILIPPA C. MADDERN. *Violence and Social Order: East Anglia, 1422-1442*. (Oxford Historical Monographs.) New York: Clarendon Press of Oxford University Press. 1992. Pp. 270. \$65.00.

The state of the public peace in fifteenth-century England and its relationship to royal authority, the

institutions of local government, and "bastard feudalism" have long generated historical debate. Philippa C. Maddern has made a significant contribution to that discussion with a book that takes for its focus one region, East Anglia, and one period, the twenty years between 1422 and 1442. For these two decades virtually complete records survive for both King's Bench and Gaol Delivery (about half of the cases in this latter court originating in indictments before justices of the peace). These records form the basis for Maddern's study.

Generalizing from this sample, Maddern argues that there is no basis for the common belief that English society in the fifteenth century was much troubled by violence. She is particularly concerned that historians not evaluate monarchy by its degree of success in maintaining public order through eliminating or containing physical violence. Far from being destabilizing, much violence in this society was highly praised. The function of the royal courts and the licit violence they practiced or sanctioned in the practice of others, in fact, was to preserve the social hierarchy and property, not to punish violence. According to Maddern, "the courts were not mere engines for the punishment or redress of wrongs; they were the forum in which fifteenth-century gentry, either in person or through their clients, proved their status—rightly or wrongly, holding property, strong or weak, influential or powerless—before the intent gaze of their peers and dependants" (p. 67). Much of the process of settling issues (and scores) took place outside of the courts, so that lawsuits were "snugly integrated into the general procedures of dispute and competition, such as arbitration and violence" (p. 68). This does not reduce the importance of courts; despite what seems their inefficiency from a modern point of view, the courts were so popular and were so energetically served, especially by the gentry, because they provided the formal, almost ritual setting in which the licit violence was sorted out from the illicit.

In an interesting section that reaches well beyond her two decades and her particular region, Maddern closely examines the medieval notion of violence itself. Justified violence runs downward through the social hierarchy, beginning at the top. God practices righteous violence against sinners, Christ suffered redemptive violence on behalf of sinners. Chivalric violence seems almost a force of nature, worthy of constant praise. Any right authority, acting with good motives against a properly wicked target (never, of course, a social superior) could blamelessly use violence. The parallel to just-war ideas is clear, and clearly noted. Because violence sustains the hierarchical order and flows downward, women, servants, and lower social layers in general were more likely to feel its effects.

Maddern is struck, however, not by the incidence of physical violence, but rather by the respect for law and the clever manipulation of legal machinery for the ends of litigants. She devotes two chapters to close

examinations of seven cases, five countryside quarrels and two instances of alleged community violence (the Norwich riots of 1437 and 1443, quarrels over jurisdiction at Bedford in 1437 and 1439). Maddern argues that investigation of such individual court cases is much more important than any fruitless effort to establish general crime statistics: these case studies reveal the norms of fifteenth-century litigants and not the assumptions of modern investigators. She concludes that much of the legal language of violence—certainly in these cases and likely in general—is merely formal and that “violence was anything but normative in the disputes and litigation by which fifteenth-century East Anglians ordered their position and affairs” (p. 166). The one regularly violent local gentlemen in the population of her case studies was, she finds, isolated and ostracized.

Of course the law had its shady side: it “was likely to be” partial. But this did not trouble fifteenth-century people, no more than the violence of “mercenary soldiers” troubled their conception of chivalry (p. 145). In this society law was held in respect “as a forum of honour, and a means to try debates” (p. 163).

Like most stimulating historical scholarship, this book raises as many questions as it answers. Restrictions of space limit me to posing only the first question that will obviously concern many historians. How widely can these twenty years of East Anglian evidence be generalized? Maddern recognizes this problem, commenting at the end of her book that “violence could either sustain or undermine authority, depending on how it was exercised. It may well be true both that in the period 1422–42, East Anglians as a whole practiced violence which affirmed social order, and that in the 1450s . . . the violent actions of magnates and their retainers helped to disrupt civil society and bring down the Lancastrian dynasty” (p. 233). In fact, the issue of generalization may be even more difficult than is suggested here. Several scholars (myself included) have advanced arguments that the late thirteenth and early fourteenth centuries, no less than the 1450s and 1460s, saw disruptive violence of a particularly virulent sort. It is hard to ignore all the evidence of violence in this slightly earlier period or to dismiss the voluminous violence as merely formulaic. Constant reform efforts by the royal administration (significant even when they were misguided or counterproductive) were advanced to meet a perceived crisis. The crown (in its private as well as its public utterances), thousands of petitioners charging violence and illegality, and writers of the well-documented complaint literature (which took injustice as one of its chief targets) all spoke the same language; all suggested a crisis of public order. One could argue, as Maddern suggests, that the norms about violence held by fifteenth-century East Anglians were actually common medieval norms, shared widely in the periods before and after her sample decades. What, then, is the relationship between these norms and violence and between violence and social order?

Are her East Anglian cases the basis for the broadest kind of generalization, or something of an exception in need of explanation?

To raise the first of a list of questions that will be provoked by this book is in no way to misperceive considerable merits. Maddern has written a clear, bold, and thoughtful work, based on a wide and meticulous search of intractable sources. Those who will disagree with her must also commend her.

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MODERN EUROPE

DAVID HENIGE. *In Search of Columbus: The Sources for the First Voyage*. Tucson: University of Arizona Press. 1991. Pp. xiii, 359. \$24.95.

David Henige addresses the current theory of textual criticism as it concerns historical documents—that is, assumptions and methods involved in establishing the text and emending it—and applies the appropriate criteria to the unique holograph abstract of Columbus's log that Bartolomé de Las Casas prepared by paraphrase and quotation from a copy of the log, and to the major editions of this holograph (usually called the *Diario* in the edited form) from Martin Fernández de Navarrete (1825) to Oliver Dunn and James E. Kelley (1989). Henige insists that the holograph should not be confused with Columbus's log itself, and that the author of the composite that the holograph records is Las Casas, not Columbus.

Henige's conclusions deny many received assumptions both about the relationship between the log and the composite recorded in the holograph, and about the quality and accuracy of the editing even in the best editions. He concludes that the words identified by Las Casas as quotations from Columbus's log by no means constitute reliable transcriptions of what Columbus wrote, and that the paraphrases are deeply colored by Las Casas's own predilections about what transpired during the voyage, especially where Columbus relates encounters with the Indians.

Second, Henige concludes that almost all the editors of the text have ignored the marginal notes that make up an indispensable part of the holograph account, and that none has recognized and used the assumptions and methods necessary to establish a sound text. Third, he argues that the procedures and results of various translators of the *Diario* sadly fail in the basic task of historical translation, which is “to elucidate what really happened” (p. 29). In reaching these conclusions, Henige painstakingly establishes relationships among three major early sources for the first voyage: the Las Casas holograph, Las Casas's *Historia de las Indias*, and Ferdinand Columbus's biography of his father, the *Historie*; and he critically examines the procedures followed by the various modern editors under consideration. Henige insists on a prime assumption of modern textual theory, to